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# Evaluation of the Medicare SELECT Amendments

## The Impact of Medicare SELECT on Cost and Utilization in Eleven States

by

A. James Lee, Ph.D.
Health Economics Research, Inc.

Steven A. Garfinkel, Ph.D.
Research Triangle Institute

Rezaul K. Khandker, Ph.D.
Health Economics Research, Inc.

Edward C. Norton, Ph.D.
University of North Carolina at Chapel Hill

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Project Officers:
Sherry Terrell, Ph.D.
Rose Hatten

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**Abstract**

OBRA 1990 permitted the marketing of Medicare SELECT products on a trial basis. SELECT products are Medigap policies that pay full benefits only when network providers are used. We evaluate the cost and utilization effects of the SELECT implementations in eleven states. In particular, we compare the before-and-after enrollment experience of Medicare beneficiaries newly enrolled in SELECT plans to the experience of those newly enrolled in traditional Medigap plans. We analyze sixteen quarters of Medicare claims data, 1991 through 1994; and we find that Medicare SELECT had no effect in two states, that it has increased costs in five states and that it has decreased costs in four states. Moreover, we find that the cost effects are substantially attributable to differences in ambulatory utilization.

## 1.0 Introduction

Medicare SELECT is a kind of individually purchased Medicare supplemental insurance policy that requires beneficiaries to use the insurer's provider network to receive supplemental benefits. Medicare benefits are unaffected by whether the beneficiary uses the supplemental insurers network (and, thus, receives supplemental benefits) or goes outside the network (and, thus, forfeits supplemental benefits). In creating Medicare SELECT, Congress expected that it would direct beneficiaries to networks of efficient providers established by the supplemental insurers. This would, in turn, reduce fee-for-service Medicare claims and enable SELECT insurers to offer beneficiaries lower premiums. This paper presents the evaluation of the impact of Medicare SELECT on Medicare program costs and beneficiary utilization.[1]

Medicare SELECT is one of several attempts to introduce managed care incentives into the fee-for-service Medicare system. Although enrollment in Medicare HMOs has grown rapidly in recent years, it still represents less than 10 percent of the Medicare population and will continue to represent a minority of the Medicare population for the foreseeable future. Thus, the growth of expenditures in the fee-for-service Medicare system will continue to be a key factor in the financial health of the Medicare program. This evaluation bears not only on the extent to which Medicare SELECT has achieved its cost containment objectives, but also contributes to the growing body of information about the prospects for managed care techniques as a way to reduce the long-term growth rate of Medicare program expenditures.

[1]The impact of SELECT on supplemental insurance premiums and on beneficiary access and satisfaction is addressed elsewhere; see Garfinkel, Lee, Khandker *et al.*, 1996).

## 2.0 Background

The Omnibus Budget Reconciliation Act (OBRA) of 1990 made two important changes in the regulation of Medicare supplemental insurance. First, OBRA 1990 imposed mandatory standards for individually purchased Medigap insurance. These standards, which took effect in 1992, limited Medigap policies to ten standard plans, labeled A through J, and guaranteed issue for Medicare beneficiaries within six months of eligibility for Medicare Part B. Second, OBRA 1990 allowed Medigap insurers in 15 states to market network-based Medigap products, called Medicare SELECT policies, on a three-year demonstration test basis.

Medicare SELECT products offer a managed care alternative to traditional Medigap insurance. In most instances, the SELECT benefits are the same as standard Medigap plans.[2] Like an HMO, however, Medicare SELECT plans pay supplemental benefits only when contracting network providers are used. Because Medicare SELECT plans are supplemental policies, they have no effect on Medicare program payments. The fee-for-service Medicare program makes its payments whether or not services are delivered in or out of the SELECT network. Thus, from the beneficiary's perspective SELECT plans function like a preferred provider organization in that when out-of-network providers are used, most of the cost (i.e., the Medicare program's obligation) is still covered, but some of the cost (i.e., the supplemental insurer's obligation) is not.

In authorizing the demonstration, legislators had anticipated that Medicare SELECT products would have lower premiums. Moreover, they thought that SELECT would help to control overall Medicare program costs because the insurers would have an incentive to establish cost-effective provider networks and then work with those provider networks to improve health system efficiency.

Fifteen states initially designated as SELECT states were Alabama, Arizona, California, Florida, Indiana, Kentucky, Michigan, Minnesota, Missouri, North Dakota, Ohio, Oregon, Texas,

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[2]Because they already had standardized Medigap plans, three SELECT states; Massachusetts, Minnesota and Wisconsin, were not required to implement the OBRA 1990 standardization. In these states, SELECT insurers were permitted to market plans that corresponded to their existing state-specific Medigap plans.

Washington, and Wisconsin. Oregon and Michigan, however, quickly withdrew due to lack of insurer interest and, in mid-1993, Illinois and Massachusetts were selected as replacements.

Although HCFA was responsible for supervising the Medicare SELECT program, actual implementation was the responsibility of the department of insurance in each state. Within the guidelines of the NAIC model legislation, each state implemented SELECT according to its own insurance regulations, procedures and standards. Moreover, the state-specific implementations were substantially shaped by the insurers themselves who were required to seek approval from the state departments of insurance before marketing a SELECT product. In our case studies of the state implementations, we found considerable variation across states in how SELECT was implemented (Lubalin, Garfinkel, Lee, *et al.*, 1994).

The original three-year demonstration, which had been scheduled to end December 31, 1994, was extended to June 30, 1995 by the Social Security Amendments of 1994 (P.L. 103-432). In late June 1995, Congress extended the authority to approve Medicare SELECT plans to all states (P.L. 104-18). Medicare SELECT will now remain in effect until June 30, 1998 unless the Secretary of Health and Human Services finds that it (1) has not resulted in savings of premium costs to beneficiaries, compared to non-SELECT Medigap policies; (2) has resulted in significant additional expenditures for the Medicare program; or (3) has resulted in diminished access and quality of care.

This study addresses the second issue, namely, whether Medicare SELECT has caused an increase in Medicare program expenditures. In addition to answering this question, we also disaggregate total Medicare program spending into its utilization and cost components and estimate the effects of SELECT on each component. Due to the variability in state program implementation, we did not pool data across states in evaluating SELECT but rather evaluated each state's SELECT implementation separately.

## 3.0 Analytic Approach

In evaluating SELECT effects on Medicare program cost and utilization, we use a "four-way" quasi-experimental design, comparing the before-and-after enrollment experience of Medicare beneficiaries newly enrolled in Medicare SELECT plans with the before-and-after enrollment experience of a matched sample of Medicare beneficiaries newly enrolled in traditional post-OBRA, Medigap plans. The beneficiaries included in the two groups were first matched, to the extent possible, by age, gender and geographic area.

We evaluate the effects of SELECT on 14 different measures of cost and utilization. Although we report results for all measures, we focus on our most comprehensive cost measure, total allowable Medicare expense (including deductibles and copayments). Thus, the greatest attention is given to answering the principal policy question, namely, does SELECT reduce total Medicare costs?

We estimate the cost models as log-linear relationships; and we estimate the utilization models as linear relationships.[3] We found that this analytic strategy gave the most robust estimates across different model types and different dependent variables. All models were estimated separately for each state.

**Fixed Effects Model.** Preliminary analyses using a simple cross section/time series design indicated potential selection bias. Specification tests showed significant differences in the Medicare costs incurred prior to enrollment in the current Medigap plan, even after controlling for observable characteristics (Heckman and Hotz, 1989). In an effort to mitigate selection bias, we used the fixed effects procedure in estimating SELECT effects. Fixed effects is the standard econometric procedure for avoiding or reducing selection bias. It controls completely for time invariant differences among individuals.

In estimating the fixed effects models, we estimated a unique intercept for each individual beneficiary. While this procedure controls for all measured and unmeasured person-specific characteristics, it also means that all time invariant variables (e.g., sex, race, location, and reason for entitlement) are excluded from the estimation and cannot be included as explicit control

[3]This specification has an error term distribution that is close to normal and reduces the influence of outliers.

variables. Indeed, this is the major limitation of fixed effects estimation, namely, that one cannot ascertain the role or importance of time invariant covariates. For this reason, we also report results from a cross section/time series model estimated without using fixed effects. We nevertheless believe that the fixed effects estimation provides the more robust and reliable estimates of SELECT effects. It provides the strongest control for self-selection as an alternative explanation for observed cost differences.

The only other variables besides the "fixed effects" (i.e., person-specific intercepts) included in the fixed effects models are the variables that vary over time. In particular, the fixed effects models include three key variables--**SELECT**, **MEDIGAP**, and **QUARTER**.

**SELECT** is our *treatment* variable, a dummy or dichotomous variable that ranges between zero and one. For SELECT enrollees, it was set equal to zero for quarters prior to SELECT enrollment. It was set equal to one for quarters after SELECT enrollment. For the quarter in which SELECT enrollment occurred, **SELECT** was defined proportionately (e.g., set equal to 0.50 if enrollment occurred midway through a quarter). For non-SELECT enrollees, this **SELECT** variable is always zero. This variable is our indicator of the SELECT program effect.

To distinguish pre-enrollment quarters from post-enrollment quarters, we used another dummy variable designated **MEDIGAP**. For all sample beneficiaries, this variable was set equal to zero for quarters prior to enrollment in a post-OBRA Medigap product, traditional or SELECT. It was set equal to one for quarters after such enrollment. For SELECT enrollees only, the **SELECT** and **MEDIGAP** variables have the same values. Thus, the **MEDIGAP** variable controls for or distinguishes the effect of enrollment in any post-OBRA Medigap product, traditional or SELECT, and the **SELECT** variable distinguishes the incremental or differential effect of enrollment in a SELECT product. Thus, for SELECT enrollees, the effects are additive.

We also included a variable, **QUARTER**, to account for time trend effects. **QUARTER** takes a value ranging from 1 through 16 depending on which of the 16 quarters in our data set is represented by the observation.[4]

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[4]A second time trend variable (**QUARTER squared**) had been included in preliminary work. However, we eventually concluded that having multiple measures of time was causing multicollinearity problems.

In exploratory estimation, we had included dummy "letter" variables for each of the NAIC standard plans to control for differences in the comprehensiveness of Medigap benefits. In some analyses, we also included several time-related interaction variables (namely, interactions of **QUARTER** with **MEDIGAP** and **SELECT**) to discriminate differences in impact over time. However, we found that the additional variables introduced substantial multicollinearity, causing the coefficient estimates to be unstable. Therefore, they were excluded in the final estimation.

Finally, three dummy variables (**SPRING**, **SUMMER** and **FALL**) were used to distinguish the seasons of the year, with Winter being the omitted category. These variables were included to control for seasonal variation in health care use.

**Cross Section/Time Series Model.** This model includes not only the variables included in the fixed effects model but also a number of time invariant beneficiary characteristics. A dummy variable (**EVER**) identifies those ever enrolled in a SELECT product. This variable distinguishes the SELECT group from the comparison group and controls for prior use differences between the SELECT and non-SELECT beneficiaries. In essence, **EVER** distinguishes the experimental group from the comparison group, **MEDIGAP** distinguishes pre-enrollment quarters from post-enrollment quarters, and **SELECT** represents the interaction of the two. Of course, we could not include **EVER** in the fixed effects model because it does not vary over time.

The following variables were also included as independent variables in the cross section/time series estimation to control explicitly for factors that might otherwise explain the differences between SELECT and comparison beneficiaries:

To control for beneficiary demographic characteristics that might be associated with health status and, thus, the use and cost of health services:

- six continuous variables (**AGE65**, **AGE70**, **AGE75**, **AGE80**, **AGE85** and **AGEGT85**) specifying age in a piecewise linear fashion,[5]

[5] **AGE65** was defined to be actual age, up to age 65; and it equals 65 for those older than 65. **AGE70** ranges between zero and five. It equals zero for those younger than 65, and it equals five for those older than 70. For those aged 65 to 70, the value is set equal to actual age minus 65. The other age variables are defined analogously, with **AGEGT85** being an open-ended category for those older than 85. This method of specifying the age variable is the standard econometric "spline" technique.

- a dummy variable (**FEMALE**) identifying those who are female,
- a dummy variable (**BLACK**) identifying those who are black, and
- a dummy variable (**OTHER**) identifying those who are other nonwhite.

To control for the beneficiary's Medicare eligibility status, which is associated with health status by definition:

- a dummy variable (**DISABLED**) identifying those who are disabled,
- a dummy variable (**RENAL**) identifying those with renal disease,
- a dummy variable (**AGED_DIS**) identifying those who are both aged and disabled, and
- a dummy variable (**AGED_REN**) identifying those who are both aged and have renal disease.

Finally, to control for geographic variation in provider availability and payment rates, we included dummy variables identifying county of beneficiary residence; and to control for insurer differences in risk selection, we included a series of dummy variables distinguishing the various SELECT and non-SELECT insurers.

Whereas cross section/time series model specification is reasonably comprehensive, it is nevertheless important to acknowledge that this basic cross section/time series model, and all other models estimated, are incompletely specified. In particular, we had no information on beneficiaries' prior supplemental insurance status. We did not know whether a given beneficiary had another Medigap product during the time interval prior to reported enrollment in a post-OBRA traditional or SELECT product. As a practical necessity, our analysis must assume that the traditional and SELECT enrollee populations had the same distribution of supplemental benefits prior to post-OBRA enrollment. To the extent that this assumption is incorrect, however, our estimates of the Medigap and SELECT impacts could reflect bias.

If, for example, SELECT (due to price advantage or market positioning) were relatively more attractive to Medicare beneficiaries without a prior supplemental plan, our estimates of the SELECT effects could have a positive bias. Health services research has consistently shown that beneficiaries with supplemental insurance have higher Medicare costs than those without (see Christenson, Long and Rodgers, 1987; Link, Long and Settle, 1980; McCall *et al.*, 1991, and Khandker and McCormack, 1996). Indeed, our own results provide additional, strong support

for that proposition. If so, a cost increasing result would be obtained if those enrolling in SELECT had been less likely to have a supplemental plan prior to post-OBRA enrollment.

Unfortunately, no comparative information was available on the prior insurance status of the SELECT and comparison beneficiaries. However, we believe that the potential for such selection bias is much diminished in those states with very large SELECT enrollment (e.g., Alabama, California and Minnesota). Their SELECT populations are much more likely to be representative of the larger Medicare beneficiary universe in those states. Furthermore, inasmuch as our findings for these three states mirror the pattern of results for all states in which SELECT is evaluated, we do not believe that the differences seen are wholly or even substantially attributable to selection bias.

**Other Models.** In addition to the fixed effects and cross section time series models, we estimated other models to check the robustness of our results. We estimated an expanded fixed effects model that also included the time invariant covariates (e.g. gender and race). We estimated a prior use model in which the dependent variable in the last year of our data is modeled as a function of the baseline value of the dependent variable in the first year of our data, in addition to other factors. We also estimated a first difference model wherein the dependent variable was constructed as the difference between experience in the first and last years of our data.

None of these models is as theoretically appealing or empirically robust as the fixed effects model. We found that although the expanded fixed effects model gave reasonably consistent estimates for the SELECT variable, the estimates for the Medigap and time trend variables were much less stable. Moreover, the estimates obtained for the time invariant variables were typically highly insignificant, because the fixed effects transformation had already removed most of the variation. Both the prior use and first difference models required that a substantial portion of the sample be excluded, because only beneficiaries enrolled in Medicare for the entire four-year study period could be included. This requirement excluded beneficiaries disenrolling from HMOs and those who became eligible for Medicare during the study period. Because newly eligible Medicare beneficiaries are the most likely group to be in the market for Medigap insurance, their exclusion significantly impairs the policy relevance of the results obtained using the prior use and first difference models.

We judge that the fixed effects model provides the best estimate of SELECT program effects. The results are more stable than the results obtained with other models; it permits inferences to the more inclusive and representative reference population; and it provides the strongest control for selection bias as an alternative explanation. Thus, we emphasize the results of the fixed effects model. Nevertheless, the other models, with possible exception of the prior use model, yield results that are broadly consistent and indicate similar appraisal of the SELECT program.

## 4.0 Data

Our analyses were limited to the 12 states with SELECT enrollment in February 1994. Illinois, Massachusetts, and Washington were excluded because they did not have SELECT enrollment at that time. In addition, we excluded North Dakota because the sample size available for this state was too small to permit reliable estimation.

For both the SELECT and comparison groups, we obtained all Medicare claims, Part A and Part B, for services provided during the four-year interval, 1991 through 1994. The utilization and cost experience was then summarized by beneficiary for each of the 16 quarters in that interval. Thus, our data include a maximum of 16 observations for each beneficiary. Moreover, for each beneficiary, our data include only those quarters for which the beneficiary was alive, continuously eligible for Medicare (both Parts A and B), and not enrolled in an HMO.

The number of beneficiaries enrolled in SELECT ranged from four to five hundred in Indiana and Ohio to more than 25,000 in Alabama, California, and Minnesota (see Table 1). The sample included only beneficiaries newly-enrolled in SELECT products. Rollovers from pre-OBRA network-based Medigap products were excluded so that the pre-SELECT period did not represent enrollment in a similar network-based Medigap product. In each state, we sought to have an approximately equal number of non-SELECT comparison beneficiaries. However, the number of non-SELECT beneficiaries in Alabama, Kentucky, and Minnesota is substantially less than the number of SELECT beneficiaries (see Table 1, third column) because the sampling frame did not include a sufficient number of newly enrolled non-SELECT beneficiaries to match the newly enrolled SELECT universe. Such imbalances reduce the precision in estimating the SELECT effects but do not bias or otherwise invalidate the findings.[6]

The average number of quarterly observations per beneficiary ranged from 10.6 to 13.6 for the SELECT sample, and from 12.0 to 13.7 for the comparison group. There are several reasons for having fewer than 16 quarters of data. First, the enrollment in post-OBRA products is

[6]A detailed description of the methods used to identify and match the SELECT and comparison groups may be found in *Evaluation of the Medicare SELECT Amendments: Final Evaluation Report* (Garfinkel, Lee, Khandker, *et al.*, 1996).

Table 1

Numbers of Beneficiaries and Average Number of Quarters Represented in SELECT and Non-SELECT Samples

| | SELECT Sample | | Non-SELECT Sample | |
|---|---|---|---|---|
| State | Number of Beneficiaries | Average Number of Quarters | Number of Beneficiaries | Average Number of Quarters |
| Alabama | 30,793 | 11.64 | 4,367 | 13.08 |
| Arizona | 1,189 | 13.03 | 1,152 | 13.66 |
| California | 38,683 | 12.49 | 31,416 | 12.40 |
| Florida | 12,393 | 13.23 | 12,145 | 13.53 |
| Indiana | 523 | 12.21 | 450 | 13.41 |
| Kentucky | 13,402 | 12.20 | 4,905 | 12.71 |
| Minnesota | 25,533 | 12.01 | 3,410 | 13.94 |
| Missouri | 4,656 | 13.17 | 3,984 | 12.75 |
| Ohio | 425 | 12.50 | 499 | 11.96 |
| Texas | 8,551 | 13.59 | 5,663 | 13.73 |
| Wisconsin | 2,339 | 10.61 | 1,695 | 12.19 |
| | **138,487** | | **69,686** | |

weighted towards those newly eligible for Medicare (i.e., those just turning age 65); and, naturally, no claims data are available for times prior to first date of Medicare eligibility. Second, for Medicare beneficiaries previously enrolled in an HMO, the Medicare program collects no utilization and cost information for the interval of HMO enrollment. In several states (e.g., Wisconsin and Indiana), a substantial proportion of SELECT enrollees were formerly enrolled in an HMO. Third, a small number of beneficiaries died during the 16-quarter period.

## 5.0 Results

**Total Cost Per Beneficiary** Our results, in Table 2, show that the effect of SELECT varies widely across states. Significant, positive (cost-increasing) estimates are obtained for five states--Alabama, Arizona, Indiana, Texas, and Wisconsin; and significant, negative (cost-decreasing) estimates are obtained for four states--California, Florida, Missouri, and Ohio. The significant, positive effects range from a low of 7.9 percent in Texas to a high of 45.9 percent in Indiana; and the significant, negative effects range from -18.9 percent in Ohio to -2.9 percent in Florida.

The simple average of all 11 state-specific estimates is +5.6 percent. Specification testing suggests that our fixed effects estimates for several states (e.g., Arizona) may be biased downwards. If so, the actual average effect could be somewhat higher than our 11-state average indicates. Nevertheless, the estimated average effect is significant at the .01 level, and the 95 percent confidence interval ranges from +2.3 percent to +8.9 percent. Excluding Indiana and Ohio, the two states with the smallest sample sizes and most extreme values, the simple average of the remaining estimates is +3.8 percent. This estimate is also significant at the .01 level. Its 95 percent confidence interval ranges from +1.5 percent to +6.1 percent. Both confidence intervals include both estimates, suggesting that they are not statistically different from each other. The results obtained using a simple cross section/time series specification are similar (see Table 3).

Weighted averages have not been developed, since we do not believe that it is appropriate to do so. Our state-specific sample sizes do not fairly reflect differences in SELECT market potential across states. The state-to-state differences in SELECT enrollment in our samples are substantially the result of differences in insurer marketing strategy and state insurance department regulatory policy rather than a reflection of the impact of SELECT managed care provisions. Moreover, the SELECT implementations varied considerably across states (Lubalin, Garfinkel, Lee, *et al.*, 1994). Because we view the 11 state implementations as 11 independent tests of the SELECT concept, the simple average is the more relevant summary statistic.

Any summary statistic that averages the results for states, including our simple arithmetic average, should nevertheless be used cautiously because the effects vary so much across states. In particular, a simple average of program effects should not be construed as an estimate of the

Table 2

Estimated SELECT Cost Impacts
Using the Fixed Effects Model

| State | Coefficient Estimates | Estimated SELECT Effect | 95% Confidence Level |
|---|---|---|---|
| Alabama | 0.152 ** | 16.4% | 11.2%, 22.7% |
| | (0.023) | | |
| Arizona | 0.140 ** | 15.0 | 3.1, 27.0 |
| | (0.053) | | |
| California | -0.086 ** | -8.2 | -10.2, -6.3 |
| | (0.011) | | |
| Florida | -0.029 # | -2.9 | -5.9, -0.2 |
| | (0.016) | | |
| Indiana | 0.378 ** | 45.9 | 19.9, 72.0 |
| | (0.091) | | |
| Kentucky | 0.008 | 0.8 | NS |
| | (0.024) | | |
| Minnesota | 0.009 | 0.9 | NS |
| | (0.037) | | |
| Missouri | -0.109 ** | -10.3 | -15.6, -5.0 |
| | (0.030) | | |
| Ohio | -0.209 * | -18.9 | -33.2, -4.6 |
| | (0.090) | | |
| Texas | 0.076 ** | 7.9 | 3.0, 12.8 |
| | (0.023) | | |
| Wisconsin | 0.137 ** | 14.7 | 3.4, 25.9 |
| | (0.050) | | |

**NOTES:**

Standard errors in parentheses.

\# -- significant at the .10 level
* -- significant at .05 level
** -- significant at .01 level

NS -- not significant.

Table 3

Estimates of SELECT Percentage Cost Impacts
Using Alternative Models

| | Quarterly | | | Two Period | |
|---|---|---|---|---|---|
| State | Cross Section/ Time Series | **Fixed Effects** | Expanded Fixed Effects | Prior Use | First Difference |
| Alabama | 10.8%** | **16.4%**** | 8.0%** | 13.4# | 19.8** |
| Arizona | 15.8* | **15.0*** | 15.1* | 43.3** | 28.2 |
| California | -9.5** | **-7.8**** | -7.0** | 8.3** | -6.1* |
| Florida | -0.4 | **-2.9#** | -3.0* | -15.0** | -4.3 |
| Indiana | 46.5** | **45.9**** | 31.8** | 29.8 | 23.6 |
| Kentucky | -3.5 | **0.8** | -0.8 | -12.2** | -11.0# |
| Minnesota | -9.3** | **0.9** | 2.5 | 4.8 | 6.3 |
| Missouri | -10.7** | **-10.3**** | -7.7** | -7.2 | -6.7 |
| Ohio | -34.6** | **-18.9**** | -13.8# | -3.0 | -18.4 |
| Texas | 1.1 | **7.9**** | 6.6** | 4.5 | 18.3** |
| Wisconsin | 35.4** | **14.7*** | 10.4* | -16.9# | 16.3 |
| *Average* | *3.8** | ***5.6**** | *3.8** | *4.4* | *6.6* |

**NOTES:**

\# Significant at .10 level.
** Significant at .01 level.
* Significant at .05 level.

national effect of the SELECT program. The SELECT states were not constructed as a representative sample of states or Medicare beneficiaries, and we have no way of anticipating how other states would implement SELECT.

**Medigap Estimates.** The coefficient on the **MEDIGAP** variable indicates the effect of enrollment in a post-OBRA supplemental product, whether it be traditional or SELECT (see Table 4). We obtain positive estimates for all 11 states, and all but one are significant. The estimates range from +7.4 percent to +29.6 percent; and the average is +19 percent. This average is significant at the .01 level and the 95 percent confidence interval ranges from 15.8 percent to 22.7 percent.

Our results for the **MEDIGAP** variable reconfirm and support findings from other studies that have consistently found supplemental insurance to be associated with increased Medicare utilization and costs. We included the **MEDIGAP** variable to control for this effect. Clearly, if we had not done so, our estimates of the SELECT effects would have been biased upwards substantially. The estimates for the **SELECT** impacts are incremental or additive to the **MEDIGAP** impacts.

**Other Results--An Illustrative Example.** For illustrative purposes, we report full results for California, for both the fixed effects and cross section/time series models (see Table 5). Again, as discussed above, time invariant terms are not included in the fixed effects model.

Both models indicate that the Medicare costs vary significantly with the **QUARTER** time trend variable. Also, both indicate that costs are significantly higher in the Spring quarter, relative to Winter. The fixed effects model additionally finds that the costs are significantly lower in the Fall.

The cross-section/time series model finds that women have higher costs. This model also indicates that Medicare costs decline with age to age 65, and then increase monotonically until age 85. We additionally find that costs are significantly lower for persons whose race is neither black nor white. While we find, as expected, that Medicare costs are substantially higher for the disabled and those with renal disease, we also find that those with dual entitlement are relatively less expensive.

**Other Cost and Utilization Measures.** The discussion above focuses on results obtained with our most comprehensive cost measure, namely, total allowable Medicare cost. However, we

Table 4

Estimated Medigap Cost Impacts Using the Fixed Effects Model

| State | Coefficient Estimates | Estimated Effect | 95% Confidence Level |
|---|---|---|---|
| Alabama | 0.179** | 19.6% | 14.0%, 25.2% |
| | (0.024) | | |
| Arizona | 0.127* | 13.5 | 1.3, 25.8 |
| | (0.055) | | |
| California | 0.225** | 25.2 | 22.5, 27.9 |
| | (0.011) | | |
| Florida | 0.220** | 24.6 | 20.7, 28.5 |
| | (0.016) | | |
| Indiana | 0.08 | 8.3 | NS |
| | (0.092) | | |
| Kentucky | 0.185** | 20.3 | 14.4, 26.2 |
| | (0.025) | | |
| Minnesota | 0.071# | 7.4 | NS |
| | (.037) | | |
| Missouri | 0.195** | 21.5 | 14.2, 28.9 |
| | (.031) | | |
| Ohio | 0.260* | 29.6 | 6.3, 53.1 |
| | (.092) | | |
| Texas | 0.148** | 15.9 | 10.3, 21.6 |
| | (.025) | | |
| Wisconsin | 0.231** | 26.0 | 14.6, 37.3 |
| | (.046) | | |

**NOTES:**

Standard errors in parentheses.

\# Significant at the .10 level
* Significant at .05 level
** Significant at .01 level

NR = Not Significant.

Table 5

Full Model Results for California

| Variable | Cross Section/ Time Series | **Fixed Effects** |
|---|---|---|
| Intercept | 4.999** | -- |
| | (0.142) | |
| MEDIGAP | 0.267** | **0.224**** |
| | (0.012) | **(0.011)** |
| SELECT | -0.100** | **-0.086**** |
| | (0.012) | **(0.011)** |
| EVER | 0.613** | -- |
| | (0.013) | |
| QUARTER | 0.028** | **0.049**** |
| | (0.001) | **(0.001)** |
| SPRING | 0.048** | **0.044**** |
| | (0.008) | **(0.007)** |
| SUMMER | 0.012 | **0.007** |
| | (0.008) | **(0.007)** |
| FALL | -0.011 | **-0.014*** |
| | (0.008) | **(0.007)** |
| FEMALE | 0.130** | -- |
| | (0.006) | |
| BLACK | -0.032 | -- |
| | (0.026) | |
| OTHER | -0.495** | -- |
| | (0.013) | |
| DISABLED | 1.214** | -- |
| | (0.030) | |
| RENAL | 4.272** | -- |
| | (0.077) | |
| AGED_DIS | -0.308** | -- |
| | (0.032) | |
| AGED_REN | -0.827** | -- |
| | (0.092) | |
| AGED65 | -0.021** | -- |
| | (0.002) | |

Table 5
(continued)
Full Model Results for California

| Variable | Cross Section/ Time Series | **Fixed Effects** |
|---|---|---|
| AGED70 | 0.092** | -- |
| | (0.002) | |
| AGED75 | 0.067** | -- |
| | (0.003) | |
| AGED80 | 0.062** | -- |
| | (0.004) | |
| AGED85 | 0.025** | -- |
| | (0.005) | |
| AGEDGT85 | 0.000 | -- |
| | (0.004) | |
| County Variables | NR | -- |
| Insurer Variables | NR | -- |
| Adjusted $R^2$ | 0.053 | **0.428** |
| N | 872,880 | **872,880** |

**NOTES:**

Standard errors in parentheses.

\# Significant at the .10 level.
** Significant at the .01 level.
* Significant at the .05 level.

NR = Not Reported.

also investigated the effects of SELECT on 13 additional dependent variables measuring less comprehensive cost and utilization outcomes (see Tables 6, 7, and 8). These component results help to validate and explain the more inclusive cost results.

Table 6 reports the SELECT coefficient estimates for various Part B cost measures; Table 7 reports the estimates for total Medicare costs by setting; and Table 8 reports the estimates for selected utilization

measures. These are the actual coefficients estimated from the model, not estimates of the percentage effect. Table 9 provides supplemental information on hospital admissions. We now consider the pattern of results for each study state.

**Alabama.** Consistent with our estimate of a significant, positive effect on total Medicare costs, we obtain significant, positive coefficient estimates on **SELECT** for all seven component cost measures included in Tables 6 and 7. Moreover, our results indicate that SELECT is associated with both increased ambulatory costs and increased inpatient costs. In Table 8, however, we find only that SELECT is associated with a greater office visit intensity. The **SELECT** variable is not significant in any of the three inpatient utilization models. This prompts us to ask, "How can SELECT increase inpatient costs without also increasing inpatient use?" Table 9 suggests an answer.

For the SELECT and non-SELECT hospital admissions in each state, Table 9 shows (1) the average DRG casemix weight, (2) the percentage admitted to a teaching hospital, and (3) the percentage in a disproportionate share hospital. For Alabama, we observe no difference in the average casemix weights, and we find that a somewhat lesser percentage of SELECT patients are admitted to disproportionate share hospitals. However, we also find that a substantially greater percentage of SELECT patients are admitted to teaching hospitals, 43 percent of SELECT patients compared to 33 percent of non-SELECT patients. Inasmuch as Medicare pays teaching hospitals additionally for direct and indirect medical education costs, otherwise similar patients (e.g., ones with the same DRG case weight) admitted to a teaching hospital are more costly.

**Arizona.** We reported above that SELECT increased Medicare costs in Arizona. However, the results in Table 7 indicate that SELECT increases Medicare costs only in the physician office setting. No significant effects on either outpatient department or inpatient hospital costs are seen. Moreover, the results

Table 6

Coefficient Estimates for Part B Allowable Costs

| State | Primary Care Physician | Specialty Physician | Ancillary Services | Total Part B |
|---|---|---|---|---|
| Alabama | 0.117** | 0.113** | 0.133** | 0.155** |
| Arizona | NS | 0.188** | 0.114* | 0.137** |
| California | -0.025** | -0.082** | -0.032** | -0.081** |
| Florida | 0.039** | NS | -0.044** | NS |
| Indiana | NS | 0.350** | 0.279** | 0.316** |
| Kentucky | NS | NS | 0.056** | NS |
| Minnesota | NS | NS | NS | NS |
| Missouri | -0.066** | -0.085** | -0.054* | -0.097** |
| Ohio | -0.142* | NS | -0.233** | -0.172* |
| Texas | 0.110** | NS | 0.039# | 0.054* |
| Wisconsin | 0.234** | 0.127** | 0.083# | 0.153** |

**NOTES:**

\# Significant at .10 level.
** Significant at .01 level.
* Significant at .05 level.

NS = Not Significant.

Table 7

Coefficient Estimates for Costs by Setting

| State | Physician Office | Outpatient Department | Inpatient Hospital |
|---|---|---|---|
| Alabama | 0.108** | 0.074** | 0.076** |
| Arizona | 0.082# | NS | NS |
| California | -0.038** | -0.054** | NS |
| Florida | NS | -0.063** | NS |
| Indiana | 0.151* | 0.374** | 0.299** |
| Kentucky | NS | NS | NS |
| Minnesota | NS | NS | NS |
| Missouri | -0.054# | -0.104** | NS |
| Ohio | -0.129# | NS | NS |
| Texas | 0.050** | 0.089** | 0.078** |
| Wisconsin | 0.238** | NS | NS |

**NOTES:**

\# Significant at .10 level.
** Significant at .01 level.
* Significant at .05 level.

NS = Not Significant.

Table 8

Coefficient Estimates for Utilization Measures

| | Ambulatory | | | Inpatient | | |
|---|---|---|---|---|---|---|
| State | Office Visits | Outpatient Department Visits | Ambulatory Surgeries | Inpatient Admissions | Inpatient Days | Inpatient Surgeries |
| Alabama | 0.080** | NS | NS | NS | NS | NS |
| Arizona | 0.115** | 0.053** | 0.029** | 0.005# | 0.084# | 0.037# |
| California | NS | -0.019** | NS | NS | NS | 0.0087** |
| Florida | -0.019# | -0.033** | NS | NS | NS | NS |
| Indiana | -0.100# | 0.085** | NS | -0.011# | NS | NS |
| Kentucky | 0.032* | -0.020** | 0.068* | NS | NS | NS |
| Minnesota | NS | NS | NS | NS | 0.046* | 0.012# |
| Missouri | NS | -0.056** | NS | -0.0023# | NS | 0.034# |
| Ohio | -0.092# | NS | NS | -0.008# | -0.233** | NS |
| Texas | NS | 0.013# | -0.031** | 0.003* | 0.045* | 0.007# |
| Wisconsin | NS | NS | NS | NS | NS | 0.016# |

**NOTES:**

# Significant at .10 level.

** Significant at .01 level.

* Significant at .05 level.

NS = Not Significant.

Table 9

Hospital and Casemix Characteristics for
SELECT and Non-SELECT Admissions, By State, 1994

| | Percentage in Teaching Hospital[1] | | Percentage in Disproportionate Share Hospital[1] | | DRG Casemix Weight | |
|---|---|---|---|---|---|---|
| | SELECT | Non-SELECT | SELECT | Non-SELECT | SELECT | Non-SELECT |
| Alabama | 42.9% | 32.5% | 59.6% | 66.5% | 1.45 | 1.45 |
| Arizona | 20.4 | 41.8 | 17.0 | 46.3 | 1.63 | 1.45 |
| California | 29.6 | 30.4 | 58.3 | 58.6 | 1.60 | 1.59 |
| Florida | 29.0 | 32.0 | 43.6 | 41.6 | 1.57 | 1.54 |
| Indiana | 15.5 | 19.4 | 80.4 | 88.2 | 1.45 | 1.66 |
| Kentucky | 43.0 | 48.8 | 24.8 | 52.9 | 1.48 | 1.47 |
| Minnesota | 44.0 | 45.1 | 18.5 | 13.4 | 1.48 | 1.46 |
| Missouri | 53.7 | 43.2 | 8.0 | 14.6 | 1.50 | 1.47 |
| Ohio | 64.1 | 72.6 | 68.9 | 38.4 | 1.50 | 1.42 |
| Texas | 19.4 | 29.8 | 59.9 | 53.8 | 1.50 | 1.43 |
| Wisconsin | 66.1 | 76.3 | 26.0 | 27.6 | 1.57 | 1.56 |

**NOTE:**

[1] Based on PPS Impact file (1993).

in Table 6 suggest that specialists and associated ancillary services are responsible for the increased ambulatory care costs, since SELECT is not found to be associated with increased primary care physician (PCP) costs.

Positive, significant coefficient estimates are obtained for all six utilization measures in Table 8, including the three inpatient use measures. This result prompts us to ask, "How can SELECT increase inpatient utilization without also increasing inpatient costs?" This question is also answered from the hospital admissions data in Table 9. For Arizona, we see that SELECT patients are both less likely to be admitted to a teaching hospital (20 percent compared to 42 percent), and less likely to be admitted to a disproportionate share hospital (17 percent compared to 46 percent). Thus, SELECT patients are being admitted to less costly hospitals, and the savings achieved on a per admission basis possibly offsets the costs of increased admissions.

We also see from Table 9 that the SELECT patients have a somewhat higher casemix index, 1.63 compared 1.45. This suggests that Arizona SELECT plans have enrolled a less favorable risk (i.e., patients requiring more costly, higher intensity care). The fixed effects procedure controls for such differences in estimating SELECT effects, but only to the extent that the risk profile differences existed prior to SELECT enrollment.

**California.** Our results above indicate that Medicare is saving money on SELECT in California. The supplemental results in Tables 6 and 7 suggest that the cost savings is coming entirely from ambulatory care. Both physician office and hospital outpatient department (OPD) costs are reduced; and the costs are reduced for PCPs, specialists and ancillary services. The OPD utilization measure (Table 8) also elicits a significant, negative coefficient. Finally, as seen in Table 9, the characteristics of SELECT and non-SELECT hospital admissions in California are similar.

**Florida.** SELECT was also estimated to reduce total Medicare costs in Florida. Like California, it appears that the Florida cost savings are coming from ambulatory care. Our results indicate both that OPD and physician visit rates are significantly reduced, (Table 8) and that OPD and ancillary costs are significantly reduced (Tables 6 and 7). Although we also obtain a negative estimate for the impact on physician office costs, this estimate is not quite significant. However, our results also suggest that PCP costs are increased. No effect on inpatient costs was indicated

As seen in Table 9, a modestly lower percentage of SELECT patients in Florida are admitted to teaching hospitals and a modestly higher percentage are admitted to disproportionate share hospitals. The average casemix weights are virtually identical for the two groups.

**Indiana.** Although the sample size for Indiana is small compared to nine of the other states, we nevertheless estimate that SELECT has a significant and sizeable cost increasing effect in this state. With the exception of PCP costs, we obtain significant, positive coefficients for all cost measures included in Tables 6 and 7.

In Table 8, however, we have an inconsistent result. We estimate that SELECT has reduced acute hospital admissions . Moreover, Table 9 is not helpful in explaining this discrepancy. Indiana SELECT patients have lower casemix weights, and are both less likely to be admitted to a teaching hospital and less likely to be admitted to a disproportionate share hospital. How then do we account for our finding that the inpatient costs are increased? One answer might be that the SELECT admissions involve more outliers. If so, this could only be attributed to time variant selection bias (i.e., selection biases that only become manifest during the study period).

Consider the following scenario. In Indiana, 15 percent of the SELECT enrollees had been disenrolled from an HMO, compared to only 1.6 percent of non-SELECT enrollees. If the HMO disenrollees were not only sicker than average, but also getting sicker at a disproportionate rate, our estimation would attribute a positive cost impact to SELECT. Comparatively little is known, however, about the health status or health status progression of HMO disenrollees. In other SELECT states, except Wisconsin,[7] we do not observe similar imbalance in the distribution of HMO disenrollees across SELECT and non-SELECT groups.

**Kentucky and Minnesota.** In both Kentucky and Minnesota, we had detected no significant effects on overall Medicare costs. Those results are generally mirrored by other findings reported in Tables 6, 7, 8 and 9. In Kentucky, we find that, although ancillary costs are increased, utilization is shifted towards more cost-effective settings. The OPD visit rate is reduced, but the office visit rate and the ambulatory surgery rate are increased. SELECT patients in Kentucky are much less likely to use a disproportionate share hospital, 25 percent of SELECT

[7]In Wisconsin , the situation in that state is qualitatively different. A staff model HMO converted its entire Medicare risk program to SELECT. Thus, for this group, selection bias should not be a factor.

admissions compared to 53 percent of non-SELECT admissions. They are also less likely to use a teaching hospital.

For Minnesota, none of the cost measures in Tables 6 and 7 elicit a significant coefficient. We find only that inpatient days and inpatient surgeries are significantly increased. SELECT patients are nevertheless somewhat more likely to use disproportionate share hospitals (see Table 9).

**Missouri.** SELECT is estimated to reduce overall Medicare costs in Missouri. Our supplemental results indicate a consistent pattern of ambulatory care cost reductions associated with SELECT (Tables 6 and 7). Only inpatient costs are not reduced, despite a small and barely significant reduction in inpatient admissions. The cost savings are achieved despite a greater use of teaching hospitals, 54 percent of SELECT admissions compared to 43 percent of non-SELECT admissions. The reduction in costs for all types of services except inpatient hospital care is paradoxical because none of the three SELECT insurers in Missouri uses a physician network. All three use hospital-only networks, which would suggest a possible change in hospital cost and use, which we do not find.

**Ohio.** We estimate that overall Medicare costs are reduced for SELECT enrollees in Ohio. Our component cost estimates suggest that the savings were achieved on primary care and related services provided in physician offices (Tables 6 and 7). In particular, we obtain significant, negative coefficient estimates for the primary care physician, ancillary services, total Part B and physician office setting cost aggregates. This reduced ambulatory care costs are consistent with the reduced physician office visits in Table 8.

We also find that the inpatient admission rates and inpatient days are both significantly reduced, which should imply reduced inpatient costs. However, we found no effect on inpatient costs (Table 7). Table 9 shows that the SELECT patients are both somewhat less likely to use teaching hospitals and substantially more likely to use disproportionate share hospitals. Evidently, the inpatient utilization reductions are offset by the higher reimbursement rates. Although it should not affect the estimation, we additionally note that the average casemix weight for SELECT admissions in Ohio is slightly higher than that for non-SELECT admissions.

**Texas.** As found in the aggregate, the Texas results for component measures provide consistent evidence that SELECT has increased Medicare costs. We obtain significant, positive

estimates for the SELECT variable on all cost measures except specialty physician services (Tables 6 and 7). Moreover, the utilization results indicate that the OPD visits, inpatient admissions, inpatient days, and inpatient surgery rate are significantly increased. The ambulatory surgery rate is reduced (Table 8). Inpatient costs are significantly higher despite the fact that SELECT patients are substantially less likely to be admitted to a teaching hospital, 19 percent of SELECT admissions compared to 30 percent of non-SELECT admissions.

**Wisconsin.** In Wisconsin, SELECT was estimated to increase aggregate Medicare costs. In Tables 6 and 7, we find evidence for cost increasing effects only in the physician office setting. No effects on OPD or inpatient costs are indicated. Moreover, PCP, specialty physician and ancillary service costs are all increased, as is total Part B costs. However, we do not find in Table 8 that the office visit rate is significantly increased. Wisconsin's SELECT patients, are less likely to use teaching hospitals but there is no difference in the use of disproportionate share hospitals or in the average casemix weights.

On balance, we believe that these supplemental analyses, using other cost and utilization measures, give results that are broadly consistent with our overall cost findings and significantly validate those results. We also suggest, based on the supplemental analyses, that the cost factors are different in different states. There seems not to be any simple explanation for either increased or reduced costs under SELECT. Like SELECT itself, the dynamic seems to vary by state. Moreover, the supplemental analyses serve to underscore the complexity of the behavioral dynamic and to establish the merit of time variant selection bias concerns in at least Indiana.

## 6.0 Discussion

Our analysis gives undeniably mixed results, with the estimated effects varying substantially by state. Five states show cost increases; four states show cost decreases; and two states show no effect. Moreover, we see no obvious patterns in the SELECT implementations that would explain the variation in findings across states. California, Florida, Minnesota, and Wisconsin are all reasonably mature managed care states. Two of these states (California and Florida) show cost decreases, one state (Wisconsin) shows a cost increase; and one state (Minnesota) shows no effect. All SELECT insurers in Indiana, Kentucky, Missouri, Ohio, and Texas use hospital-only provider networks. Two of these states (Indiana and Texas) show cost increases; two (Missouri and Ohio) show cost decreases; and one (Kentucky) shows no effect. SELECT products were based on pre-OBRA network products in Alabama, California, Minnesota, and Wisconsin. Two of these states (Alabama and Wisconsin) show cost increases; one (California) shows a cost decrease; and one (Minnesota) shows no effect.

Although significant inpatient effects were indicated in three states (Alabama, Indiana, and Texas), we find that the cost effects predominantly reflect differences in ambulatory care costs. The estimated effects on physician office costs are significant in eight of the 11 study states; and the estimated effects on OPD costs are significant in six states.

In general, we believe that our fixed effects results reflect actual SELECT program effects and cannot be easily attributed to either selection or specification biases. We do not, however, preclude the prospect that biases of one kind or another have skewed our estimates in one or more states. In particular, we are concerned that we did not know the Medigap insurance status of beneficiaries prior to purchase of their current policy. We are also concerned that the estimates for Indiana do not reflect true SELECT program effects because the effect is so large and the rate of transfer from Medicare HMOs is so much higher among SELECT beneficiaries than among comparison group members. On the other hand, we think it unlikely that analytic biases could explain the overall pattern of results. Indeed, the mixed nature of our findings tends to make the estimates all the more credible, since it is difficult to posit any other explanation.

The original premise of SELECT had been that it would reduce aggregate health care costs because SELECT insurers would have an incentive to establish cost-effective provider

networks and then support the networks in improving health system efficiency. The case study found that, as implemented by most insurers, SELECT is a comparatively weak form of managed care (Lubalin, Garfinkel, Lee, *et al.*, 1994). Many SELECT insurers do not include physicians in their provider networks, choosing instead to recruit hospitals that discount or waive the Part A deductible and to cover the Part B deductible and coinsurance for any physician that the beneficiary decides to use. Most insurers that use physician networks organize them as preferred provider networks without gatekeepers, again, a relatively weak form of managed care. Thus, on the basis of the case study, we had anticipated finding little, if any, effect of SELECT on utilization or costs.

How, then, does one account for the finding that the SELECT plans in several states have apparently increased health care costs? What are the potential mechanisms for effecting such cost increases? We offer three potential explanations.

Like some PPOs, some SELECT plans may have contracted with providers on a discounted fee basis and not given sufficient attention to managing the overall efficiency of health care services. In some PPOs, the providers simply recouped their discounts by providing or billing more services. In other instances, the PPOs had, in contracting on a percentage discount basis, unwittingly selected the more costly providers (i.e., the ones with greater margins and thus greater flexibility to accept a discount). Whatever the mechanism, employers found that the PPOs were actually costing them more, much as we are finding with regard to the SELECT experience in several states.

The possibility that SELECT insurers unwittingly chose more expensive providers may explain why SELECT affected physician costs in Indiana, Missouri, Ohio, and Texas even though none of the SELECT networks in those states included physicians. The physicians associated with the SELECT network hospitals may be systematically more or less expensive than other physicians used by the comparison population. We encountered no evidence that the ambulatory utilization patterns of physicians affiliated with network hospitals was considered by insurers when they developed hospital-only networks. However, such differences would explain the impact of hospital-only SELECT plans on physician costs, if SELECT beneficiaries were more likely to use physicians affiliated with hospitals in their SELECT network. If this pattern occurred by chance rather than by design, we would expect to see a cost-increasing impact in some cases

(e.g., Indiana and Texas) and a cost decreasing impact in others (e.g., Missouri and Ohio), as we have found.

Finally, in Wisconsin, a predominantly rural state, the increased costs could arise due to SELECT's role in improving health care access. In many rural and other underserved areas, Medicare risk-contracting HMOs have found that they are unwilling or unable to provide Medicare services within the AAPCC experience-based capitation. They argue that access barriers have impeded health care use by the fee-for-service population and left traditional Medicare beneficiaries with untreated or inadequately treated problems. They further argue that beneficiaries who join a multispecialty HMO receive more intensive and expensive treatment than they otherwise would have received from community providers under fee-for-service Medicare. Consequently, the costs of care provided to SELECT beneficiaries by HMO physicians would be higher than the costs for comparison beneficiaries served by non-HMO community physicians. If, as the HMOs contend, this pattern reflects poor access among rural Medicare beneficiaries not served by HMOs, the higher costs may be justifiable. On the other hand, if the difference arises due to multispecialty physicians delivering unnecessarily intensive care, the higher costs are not justified. In Wisconsin, more than half of the SELECT beneficiaries came from three multispecialty staff-type HMOs that had terminated their risk arrangements with Medicare because they perceived that they could not afford to provide care on a community-rated basis. This is consistent with a hypothesis that the SELECT beneficiaries served by these HMOs receive more intensive treatment than comparison beneficiaries living in the same areas, although it does not address the question of whether the greater intensity is desirable.

A fourth possible explanation, namely, the hypothesis that SELECT products increase costs in some states by increasing use of high cost teaching and disproportionate share hospitals, is not consistently supported. Higher inpatient hospital costs were associated with higher total costs in only three of the five cost-increasing states (Alabama, Indiana, and Texas). Only in Alabama were higher inpatient hospital costs associated with greater use of teaching and disproportionate share hospitals. In Indiana and Texas, higher hospital costs were associated with lesser use of teaching and disproportionate share hospitals.

Much as Mathematica found in evaluating the early Medicare risk contracts (Brown, Clement, Hill, et. al., 1994), we find evidence that the early implementations of Medicare

SELECT (in some instances) have actually increased Medicare program costs. It is possible that as Medicare SELECT matures, the successful efforts of some plans may be emulated by others and SELECT may contribute to a reduction in Medicare program costs. The SELECT model first anticipated by Congress, in which insurers select efficient networks of physicians and hospitals, may offer the best opportunity for program savings. However, it has not consistently resulted in savings in the demonstration. Furthermore, most of the insurers recently entering into the SELECT market have adopted the hospital-only network model (Han *et al.*, 1996), which offers less potential for savings.

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